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Vicia leguminibus solitariis deorsum flexis hirsutis. Sauv. Monspeliens. 235.

1750 Ulmus folio latissimo scabro. Ger. Emac. 1481.

Latiore folio. Park. 1404.

XXX. *Remarks on the Opinion of Henry Eeles, Esq; concerning the Ascent of Vapour, published in the Philosoph. Transact. Vol. xlix. Part i. p. 124. By Erasmus Darwin, M. D. Communicated by Mr. William Watson, F. R. S.*

To Mr. William Watson, F. R. S.

S I R,

THE inclosed papers were designed for the perusal of the Royal Society; being an endeavour to confute the opinion of Mr. Eeles about the ascent of vapours, published in the last volume of their Transactions. But the author, having no electrical friend, whose sagacity he could confide in, has at length prevailed upon himself to be so free to send them to Mr. Watson; to whom the world is so much indebted for the advancement of their knowledge in electricity.

Whence, Sir, if you should think, that these papers have truth, the great Diana of real philosophers, to patronize them, you will confer a favour upon me, by laying them before that learned Body. If, on the contrary, you should deem this confutation trifling

or

or futile, I hope you will be humane enough to suppress them, and give me your objections; and by that means lay a still greater obligation on one, who has not the pleasure to be personally acquainted with you. From,

SIR,

Your very humble Servant,

March 23. 1757.

Erasmus Darwin,
Physician at Litchfield, Staffordshire.

LETTER I.

*To the very honourable and learned the PRESIDENT
and MEMBERS of the Royal Society.*

Gentlemen,

Read May 5. 1757. **T**HERE is ever such a charm attendant upon novelty, that be it in philosophy, medicine, or religion, the gazing world are too often led to adore, what they ought only to admire: whilst this vehemence of enthusiasm has generally soon rendered that object contemptible, that would otherwise have long laid claim to a more sober esteem. This was once the fate of chemistry: the vain and pompous boasts of her adepts brought the whole art into disrespect; and I should be sorry, if her sister electricity should share the same misfortunes. It is hence the ingenious Mr. Eeles will excuse me, for endeavouring to lay before you my opinion on the ascent of vapours, tho' it by no means coincides with that he is so strenuous to establish, and plucks a plume from his idol goddess electricity.

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I i

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The probability, supporting the hypothesis of Mr. Eeles, according to his own expressions, rests on this : “ That every particle of vapour is endued with a portion of electric fire ; and that there is no other sufficient cause assigned for their ascending.” (*Phil. Trans. vol. xlix. part. i. p. 134.*). My design is therefore first to attempt to shew, that another theory, founded on principles better known, will sufficiently explain the ascent of vapours : and then, that some kinds of vapours are not endued with a more or less than their natural share of electric æther.

The immense rarefaction of explosive bodies by heat, depends either on the escape of air before condensed in them, or on the expansion of the constituent parts of those bodies. This distinction has not been sufficiently considered by any one to my knowledge ; nor shall I at present amuse the Society upon this head ; it being enough for my present purpose to observe, that they may be thus distinguished : where air is emitted, it cannot be condensed again into the same bulk by cold ; but the expansion of heated parts of bodies, as soon as that heat is withdrawn, ceases to exist.

Nitre comes under the first of these classes : in detonation it emits great quantities of air, not afterwards condensable to the like space. This may be seen by firing a few grains of gunpowder in an unblown bladder, or in a vessel nearly full of water with its mouth inverted. The same is true of all the solid parts of animals and vegetables, when subjected to fire ; as appears from the experiments of that learned philanthropist, Dr. Hales.

But of water the contrary is evident. In the steam-engine,

engine, a jet of cold water, we find, instantly condenses that immense rarefaction ; which I apprehend could not be, if it was constituted of escaped elastic air. And altho' this steam must be acknowledged to put on some properties of air ; such as ventilating a fire ; or that a taper blown out by it, is capable of being again lighted immediately, and that without a crackling noise, which occurs when touched with water ; this does not in the least invalidate our opinion, tho' it has certainly conduced very much to propagate the former one : since from this way of reasoning, the whole must be air, and we should have no water at all in vapour.

From considering this power of expansion, which the constituent parts of some bodies acquire by heat ; and withal, that some bodies have a greater affinity to heat, that is, acquire it sooner and retain it longer than others ; which affinity appears from experiments, and which, I apprehend, is in some ratio of their specific gravities and their powers of refraction, reflexion, or absorption of light ; or at least in some ratio much greater than that of their specific gravities alone. From considering these, I say, many things, before utterly inexplicable, became easily understood by me. Such as, Why when bismuth and zinc are fused together, and set to cool, the zinc, which is specifically heavier, is found above the bismuth ? Why the buff covering of inflammatory blood, the skum of heated milk, the sedative salt of borax, which are all specifically heavier than the liquids in which they are formed, are still formed at the surface of them ? How benzoin, sulphur, and even the ponderous body mercury, may be raised into vapour, again to be condensed unaltered ?

And, lastly, how water, whose parts appear from the æolipile to be capable of immeasurable expansion, should by heat alone become specifically lighter than the common atmosphere, without having recourse to a shell inclosing air, or other assistant machinery? and when raised, I am persuaded we shall find, that to support them floating, perhaps many days, in the atmosphere, is not a knot so intricate, as to oblige us to conjure up a new divinity to unravel it.

But before we proceed to this second part of our task, it will be necessary previously to consider first, how small a degree of heat is required to detach or raise the vapour of water from its parent-fluid. In the coldest day, I might say the coldest night, of winter, when the weather is not frosty or very damp, wet linen or paper will become dry in the course of a few hours. A greater degree of heat must indeed cause a quicker evaporation. But I am persuaded, that was it not for the pressure of the superincumbent fluid, greatly less than that of boiling water would instantly disperse the whole so heated into vapour.

Secondly, That in the opinion of Sir Isaac Newton, well illustrated by the late lamented Mr. Melvil, the sun-beams appear only to communicate heat to bodies by which they are refracted, reflected, or obstructed; whence, by their impulse, a reaction or vibration is caused in the parts of such impacted bodies.

This is supported by the experiment of approaching some light body, or blowing smoke near the focus of the largest glasses; and from observing, that these do not ascend, it is evident the air is not so much as warmed by the passage of those beams thro' it, yet would instantly calcine or vitrify every opaque

opaque body in nature. And from this we may collect, that transparent bodies are only heated at their surfaces, and that perhaps in proportion to their quantity of refraction: which will further give and receive illustration from those very curious experiments, of producing cold by the evaporation of liquors, published by the learned Dr. Cullen, in the late volume of Essays Physical and Literary, at Edinburgh. In these experiments a spirit-thermometer was immersed in spirit of wine, and being suddenly retracted, was again exposed to the air; and as the spirit of wine adhering to the glass evaporated, the spirit contained within the thermometer was observed to subside. Now as the difference of the refraction of spirit of wine and glass is exceedingly minute, compared with the difference of refraction of spirit of wine and air; we may consider, in the above experiment, the heat to be communicated to the thermometer only at its surface: but here the adherent fluid escapes as soon as heated; by which means the glass, and its contents, are deprived of that constant addition of heat, which other bodies perpetually enjoy either from the sun-beams immediately, or from the emanations of other contiguous warmer bodies; and must thence, in a few minutes, become colder than before.

The ingenious Mr. Eeles, I dare say, has already foreseen the use I am going to make of this principle; *viz.* "That the little spherules of vapour will thus, " by refracting the solar rays, acquire a constant " heat, tho' the surrounding atmosphere remain " cold." And as from the minuteness of their diameters, if they are allowed to be globules, they must do

do this to a very great degree, I apprehend none of those objections will take place against us, with which Mr. Eeles has so sensibly confuted the former received theories on this subject.

If we are asked, how clouds come to be supported in the absence of the sun? it must be remembered, that large masses of vapour must for a considerable time retain much of the heat they have acquired in the day; at the same time reflecting, how small a quantity of heat was necessary to raise them; and that doubtless even a less will be sufficient to support them, as from the diminished pressure of the atmosphere at a given height, a less power may be able to continue them in their present state of rarefaction; and, lastly, that clouds of particular shapes will be sustained or elevated by the motion they acquire from winds.

I should here have concluded this paper, perhaps already too long; but upon revising it, I find, where the affinity of some bodies with heat is mentioned, that the deductions made from thence are not sufficiently explained to be intelligible. First then, If the power of expansion of any two bodies, by heat, be in a greater proportion than their specific gravities, then will there be a certain degree of heat, in which their specific gravities will be equal; and another, in which the gravity of that, which was lighter when cold, will exceed the gravity of that, which was heavier when cold. Hence zinc and bismuth alter their specific gravities in fusion; some urine, and many solutions of solids, grow turbid as they cool; others alter their colours. Secondly, If (the power of expansion by heat being equal) the power of retaining

taining heat be in a greater ratio than the specific gravities ; then, during the time of cooling after being sufficiently heated, there will be an instant, when the heavier body will become the lighter, and swim upon the other. This seems the case in the buff covering of inflamed blood, the skum of heated milk, and the crystallization of some salts : for if these effects were from the evaporation of the thinner parts at the surface, they should happen during the greatest evaporation, or when boiling ; but, on the contrary, they are all done in the greatest degree when the liquor has for some time began to cool. Lastly, If the quickness of acquiring heat be in a greater proportion than their specific gravities (the power of expansion being equal), then, during the time of their acquiring heat, there will be an instant, when the body, that was heavier when cold, will now become the lighter. From one or more of which principles, I apprehend, the volatility or fixity of all minerals, and many other bodies, takes its origin.

It is no part of my design to account to you, gentlemen, in what manner such an expansion of the parts of bodies can be brought about by the action of fire. Tho' perhaps a rotatory motion only of each particle on its own center might be sufficient to produce such a rarefaction ; and the more so, if such parts were any other figures than spheres, as by the percussion of their angles they must result further from each other. Nor is the existence of such a rotatory motion without some probability, when we observe the verticillary motion given to charcoal-dust thrown on nitre in fusion, or the wonderful agitation of the parts of burning phosphorus, or even of a common

common red letter-wafer touched by the flame of a candle. But as in this paper I have laboured (and I hope not without success) to shew you, that some properties of solar heat are sufficient to account for the elevation and support of vapours ; so in another letter I propose nearly to demonstrate to you, that the electric æther is far from having any share in the production of this important phænomenon.

From,

GENTLEMEN,

Litchfield,
Mar. 20. 1757.

Your very humble Servant,

Erasmus Darwin.

LETTER II.

*To the very honourable and learned the PRESIDENT
and MEMBERS of the Royal Society.*

Gentlemen,

Read May 5, 1757. **E**VERY theoretical inquiry, whose basis does not rest upon experiments, is at once exploded in this well-thinking age ; where truth, under your patronage, has at length broke thro' those clouds, with which superstition, policy, or parade, had overwhelmed her. But experiments themselves, gentlemen, are not exempted from fallacy. A strong inventive faculty, a fine mechanic hand, a clear unbiased judgment, are at once required for the contrivance, conduct, and application, of experiments ; and even where these are joined (such

(such is the condition of humanity!) error too frequently intrudes herself, and spoils the work.

My very respectable antagonist, Mr. Eeles, to whose ear, I am convinced, the voice of truth is more agreeable than that of applause, will forgive me the following critique on his performance; as by that means, I am persuaded, the probability of his notions will be intirely destroyed, and the foregoing theory receive additional supports.

For this purpose our first endeavour will be to shew the uncertainty of some of the most material principles, that support his arguments; and afterwards, the fallacy of the experiments he has given us.

First then, in page 130. Mr. Eeles has asserted, that the greatest possible rarefaction of water is when it boils. I think it might be said, with equal propriety, that the greatest rarefaction of solids was when they began to melt: and this may indeed be verbally true, if we chuse to alter the names of bodies, when they undergo any alteration by fire: so solids take the name of fluids, when they are in fusion; and water the name of vapour, when it is greatly rarefied in the steam-engine. Whence we find this assertion seems to be founded on a confusion in terms, and the fact far from being existent in nature.

In page 133. the sphere of electrical activity is said to be increased by heat. If by electrical activity is here meant an increase of its repulsive power (the thing, which seems to be wanted in Mr. Eeles's hypothesis), I know no experiment to show it. If it be meant, that it is capable of being attracted to a

greater distance ; I conjecture it may, as the heat will rarefy the ambient air, and we know the electric æther is attracted at very great distances in *vacuo* ; but this cannot properly be called an increased activity of electric fire.

We are afterwards told (page *ib.*) " that electric fire will not mix with air :" whence, in the succeeding section, it is argued, " That as each particle of vapour, with its surrounding electric fluid, will occupy a greater space than the same weight of air, they will ascend." In answer to this, it must be observed, that there are some bodies, whose parts are fine enough to penetrate the pores of other bodies, without increasing their bulk ; or to pass thro' them, without apparently moving or disturbing them. A certain proportion of alcohol of wine mixed with water, and of copper and tin in fusion, are instances of the first of these ; the existence and passage of light thro' air, and, I am persuaded, of electric fire, are instances of the second.

To illustrate this, the following experiment was instituted. A glass tube, open at one end, and with a bulb at the other, had its bulb, and half way from thence to the aperture of the tube, coated on the inside with gilt paper. The tube was then inverted in a glass of oil of turpentine, which was placed on a cake of wax, and the tube kept in that perpendicular situation by a silk line from the cieling of the room. The bulb was then warmed, so that, when it became cold, the turpentine rose about half-way up the tube. A bent wire then being introduced thro' the oil into the air above, high electricity was given. The oil did not appear at all to subside :

whence I conclude, the electric atmosphere flowing round the wire and coating of the tube above the oil, did not displace the air, but existed in its pores.

This experiment I formerly tried various ways, as I had conceived, if the electric matter would displace air, it might have been applied to answer the end of steam in the steam-engine, and many other great mechanical purposes. But as from the above it appears, that the contrary is true, it is evident, that electric matter surrounding particles of vapour must, in fact, increase their specific gravity, and cannot any-ways be imagined to facilitate their ascent.

I may add further, that if this be true, that it pervades the pores of air, its specific levity cannot, by any means I know, be compared with that of air. Its particular attraction to some bodies, at least to much the greater part of the terraqueous globe, is abundantly greater than that of air to those bodies: and hence its gravitation to the whole globe would appear, at first view, to exceed that of air. But the more I consider this, the more perplexing and amazing it appears to me: and thence must leave it to the investigation of my very ingenious antagonist, or some other able philosopher.

I come now to the experiments, that are given us to show all vapour to be electrified. In these Mr. Eeles seems to have been led into error, by not having observed, that many bodies electrified will retain that electricity for some time, altho' in contact with conductors. The Leyden phial may be touched three or four times by a quick finger before the whole is discharged. Almost all light dry animal or vegetable substances, such as feathers and cork, do this in a

much greater degree: and in general I have observed, the more slow any bodies are to acquire electricity, the more avaritious they are to keep it.

Part of the plume of a feather, hanging to a green line of silk about a foot long, which was suspended from the midst of an horizontal line of the same, about four yards in length, was electrified with a dry wine-glass, according to the method of Mr. Eeles; and, after being touched nine times with my finger, at the intervals of two seconds of time, still manifested signs of electricity, by being attracted at the tenth approach of it.

A cork ball, on the same line and circumstances, after being electrified, was touched at the intervals of ten seconds repeatedly, for seven times, before it was exhausted. The fumes of boiling water were conveyed upon this ball after being electrified; and, after a fumigation for thirty seconds, it shewed signs of electricity, by being attracted to the approaching finger; and, after thirty seconds more, without any fumigation, it again obeyed the finger; and again, after thirty more, but at less and less distances. The same appearances occurred to me from the fumes of resin. From whence I apprehend, that Mr. Eeles, having dipped the electrified down of the *juncus bombycinus* in vapour for perhaps half a minute (for no time is mentioned), and finding it still retained its electric attraction, was not aware, that this same had happened, if he had by intervals touched it with his finger, or any other known conductor of electricity.

As Mr. Eeles had here objected, that there was no real opposition in the electric æther of glass, and that from wax; the common experiment to shew this was

was many times repeated with constant success; *viz.* the cork ball, suspended as above, after being electrified by the wine-glass, and repelled from it, was strongly attracted by a rubbed stick of sealing-wax; and *vice versa*. In the same manner I observed the electric æther from a black silk stocking (which was held horizontally extended by the top and foot, and, being rubbed in the midst with an iron poker, was applied to the cork ball), to be similar to that of glass, and opposite to that of wax. But the following experiment appears to me to put this matter out of all doubt, and to demonstrate, that this difference is only a *plus* and *minus* of the same specific æther, and not different qualities of it, as Mr. Eeles would suppose.

A stick of dry sealing-wax was rubbed on the side of a dry wine-glass, and a cork ball, suspended as in the former experiments, played for some time between them: but glass rubbed with glass, or wax with wax, did not manifest any electric appearance. Whence it would appear, that in rubbing glass and wax together, the glass accumulated on its surface the identical æther that the wax lost. Nor is this a digression from my design: for if this opposition of the electricity of glass and wax be established, it still contributes to demonstrate the fallacy of Mr. Eeles's experiments.

But what alone would intirely destroy this electric hypothesis, is, that from the experiments of Mr. Franklin and others, the clouds are sometimes found to be electrified *plus*, sometimes *minus*, and sometimes manifest no signs of electricity at all. Whence to say an accumulation of electric æther supports these clouds,

clouds, seems an assertion built upon a very unstable foundation, whose whole superstructure may well enough be termed an air-built castle, the baseless fabric of a vision.

Add to this, that Mr. Eeles, in page 140. tells us, that himself has passed thro' clouds resting on the sides of mountains. Ought not those clouds to have immediately discharged their electricity, and fallen? And common experience may remind us, that any cold bodies will condense vapour, whatever be their electric properties. So mirrors, or the glass of windows, in damp rooms, are most frequently found covered with dew; which, of all other bodies, ought most to be exempted from collecting vapours supported by electricity, as they are the least capable to attract or draw off that æther.

From all which, well examined, I am persuaded, gentlemen, you will be induced to conclude, that tho' clouds may sometimes possess an accumulation of electricity, yet that this is only an accidental circumstance, and not a constant one; and thence can have no possible influence either in the elevation or support of them. I am,

GENTLEMEN,

Your very humble Servant,

Litchfield,
March 23. 1757.

Erasmus Darwin.